be described in a few sentences extracted from the preface:—

> "This Society exists to teach the elements of Physical Geography and Geology direct from Nature without preliminary study from books. . . . The field work has been led up to by short courses of winter lectures given in London, designed to connect together the observations to be made in the succeeding summer, and to connect the geology of the district to be examined with that of other areas."

The excursions are described in the notes written by students in the field; the lectures are reported (from shorthand notes) by Mr. White, one of the class. As regards the former, Prof. Seeley states that "students have been free to report what they saw and what they heard, and they have severally written in their own ways both as to length and language used." The lectures also "were not constructed with a view to being reported, nor were the reports written out with a view to being printed." Prof. Seeley has, however, "read the proof to remove serious inaccuracies." The lectures need no apology, for they are excellent examples of that clear and suggestive method of teaching of which Prof. Seeley is a master. The reports of the excursions also acquire a certain freshness as recording the impressions of novices, and may on that account be even more helpful to beginners than if they had been written by more experienced observers. One or two inaccuracies, however, appear to have escaped the Professor's watchful eye. Is not the statement on p. 18, relating to the presence of *Paludina* and *Unio* in such Wealden Limestones as the Petworth Marble, a little misleading? for it implies that the latter genus is common in these deposits, which, we believe, is not the case. A sentence on p. 29 suggests that "enormous pressure" is requisite to convert a sandstone into a quartzite. Very probably this would be the result, but there are not a few quartzites which show no signs of having been specially subjected to pressure. Also, it is hardly correct to call Lydian stone an altered sandstone. Again, more than once it is intimated that gneiss and crystalline schists occur in Belgium. This, if the terms be used in their ordinary sense, is incorrect; and even the porphyroids and amphibolites, and the abnormal rocks of the Bastogne district, the vague descriptions of which may have given rise to this misconception, are of extremely limited extent. But these are very trifling blemishes, which can be readily removed in a second edition. The book will be of great use to all students, living in or about London, in helping them to use their eyes; and most of all because, to quote Prof. Seeley's words, "It here and there touches upon problems which are not usually presented to beginners." But, as he rightly urges, these problems—namely, the application of stratigraphy to the elucidation of the physical geology of past epochs—"should never be absent from the mind of anyone who considers geological facts in the field."

T. G. B.

OUR BOOK SHELF.

Katalog der Bibliothek der Deutschen Seewarte zu Hamburg. (Hamburg, 1890.)

VARIOUS notices have from time to time appeared in NATURE relating to the German Naval Observatory at Hamburg, describing the building, its equipment of instruments, and the important work which is carried on there chiefly in the interests of the German Imperial and mercantile navies.

As this institution is possessed of a library containing some 10,660 works, it has for some time past been a matter of urgent necessity that an accurate and well-considered form of Catalogue should be printed and published. The required book was completed last year, and is now available.

This Catalogue shows that the library contains a large proportion of works either directly of a naval character, or bearing upon naval matters, whilst several other branches of science are fairly represented.

As might be expected, meteorology holds the first place of importance, and amongst the 2769 works on this subject are a large proportion of Dove's writings. Indeed, it seems worthy of note that Dove's library, which occupied him many years in collecting, may now be found at the German Naval Observatory. Turning to the division of the Catalogue on physics, 1617 works will be found; on magnetism and electricity, 974; whilst other subjects, such as navigation, hydrography, and construction of ships are well cared for.

Although the books and papers mentioned in this Catalogue are generally printed in the language adopted by their authors, a translation into German of several works of interest is also placed side by side with the original.

In conclusion it may be remarked that although there is nothing specially new in the arrangement of this book, it is well worthy of the time and energy which have evidently been spent in bringing the work to its present state.

Scientific Results of the Second Yarkand Mission; based upon the Collections and Notes of the late Ferdinand Stoliczka, Ph.D.—Coleoptera. By H. W. Bates, F.R.S., J. B. Baly, D. Sharp, F.R.S., O. Janson, and F. Bates. Pp. 1-79 and 2 Plates. (Calcutta: Published by order of the Government of India, 1890.)

THIS, the twelfth part issued, all but one of which deal with zoology, contains an enumeration of 207 species of Coleoptera. These species belong to the following families:—Cicindelidæ (4), Carabidæ (60), Longicornia (5), Phytophaga (25), Haliplidæ (1), Dytiscidæ (8), Gyrinidæ (1), Hydrophilidæ (3), Staphylinidæ (9), Scarabæidæ (38), Cetoniidæ (3), and Heteromera (50). Diagnoses or descriptions of all the new genera and species were published more than ten years ago, and the only additional information contained in this part is a list of species, in addition to, in some cases, fuller descriptions of the novelties. In the portions contributed by Mr. H. W. Bates and Dr. Baly, both of whom, however, give some particulars regarding geographical distribution, the references to the published diagnoses are given; but in Dr. Sharp's and Mr. F. Bates's contributions, many of the genera and species are mentioned as new, though diagnoses of the whole of them were published in 1878 or 1879—the former in the Journal of the Asiatic Society of Bengal, xlvii. Part 2 (1878), the latter in *Cistula Entomologica*, ii. (February 1879). The two plates include 44 figures—Carabidæ (17), Longicornia (5), and Heteromera (22). On the cover, and also on p. 37, the name "Hydrophilidæ" is misprinted "Hydroptilidæ." The Hydroptilidæ do not belong to the order Coleoptera at all, but to the Neuroptera! It is to be regretted that a delay, the cause of which is not explained, of more than ten years, has occurred in the publication of the "Part" dealing with the Coleoptera, as works of this kind upon the beetle fauna of little-known districts are always of the highest value, more particularly in the matter of geographical distribution. No systematic work upon the Coleopterous fauna of India has

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yet been published, and even a fragment like the present, containing a list of the species of a neighbouring region, is a welcome addition to our knowledge. Four other "Parts" have been issued on the Insecta—the "Neuroptera" and "Hymenoptera" (both in 1878), and the "Lepidoptera" and "Rhynchota" (both in 1879); the last Part of the whole series being the "Araneidea" (1885).

Popular Astronomy. By Sir George B. Airy, K.C.B. Seventh Edition. Revised by H. H. Turner, M.A., B.Sc. (London: Macmillan and Co., 1891.)

ALTHOUGH our astronomical knowledge has been enormously extended since the lectures forming the basis of this well-known book were delivered (1848), Mr. Turner has not found it necessary to make any very considerable revision, for the reason that the advances have been chiefly on the chemical and physical sides. Still, in the lapse of time, methods of observation have been improved, and accounts of these find a place in Mr. Turner's notes. Among these are short descriptions of the chronograph and the new "electrical controls" for the driving-clocks of equatorials. One of the most noteworthy points brought out in the new edition, however, is the modern estimate of the value of observations of the transit of Venus as a means of determining the solar parallax. It was formerly supposed that this would be one of the best methods, but the difficulties encountered in 1874 and 1882 prevented observations of the necessary degree of accuracy; and now most astronomers are of opinion that this method can never give more than an approximation to the truth. Numerous minor additions have also been judiciously made.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Force and Determinism.

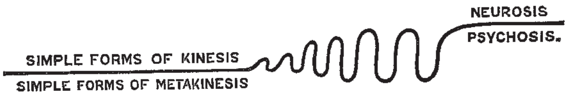
"THE relation between force, which is a mechanical thing, and will or life, or whatever it is, which is a psychological thing"—a relation which, as Dr. Lodge rightly says, "demands investigation"—presents itself to some of us as follows.

When a stimulus received by an organism gives rise to a response, however particular to the individual respondent, there are (1) a number of complex but determinate molecular changes in the organic tissues; and (2), accompanying some of these changes, certain psychological states. Are these psychological states produced by the molecular changes? or are the molecular changes produced or in any way guided by the psychological states? Neither the one nor the other. The molecular changes and the psychological states are different aspects of the same occurrences. In other words, they are distinguishable (and the distinction is absolute), but not divisible.

"The energy displayed by a gang of navvies is not theirs, but their victuals'; they simply direct it." In physiological language it is the outcome of the proper functioning of their cerebral control-centres. Now we believe that, although we can at present by no means adequately explain them, all the molecular occurrences within the organism, forming, as we believe they do, an orderly and determinate sequence between stimulus and response, whether they involve force or energy, are of such a nature as to be explicable in physical and physiological terms. The fact that certain phases of the sequence have also a subjective or psychological aspect does not, it is held, justify us in changing our point of view, and ignoring the distinction between the psychology and the physiology of the process.

Now to say that mind, or will, or consciousness directs the organic energy along a definite path we regard as incorrect, because it ignores a distinction which we hold to be valid and valuable, and conducive to clear thinking on these difficult subjects. But we have no such objection to the statement that the energy is guided by molecular forces which have for their subjective aspect certain states of consciousness. To unscientific folk this may sound mere quibbling; but to physicists, who have done so much to teach us the vital importance of accurate language for clear thinking, we look for support in drawing this distinction, unless the distinction can be shown to be either invalid or useless.

This distinction between force, energy, and the physical series (what I have elsewhere spoken of as *kinesis*) on the one hand, and thought, consciousness, and the psychical series (what I have elsewhere spoken of as *metakinesis*) on the other hand, we hold to be absolute; while at the same time we hold that consciousness is indivisible from particular (neural) modes of kinesis. And this distinction we hold to be especially valuable when questions of the origin and development of consciousness are under consideration. This may, perhaps, best be expressed by a diagram.



Now, looked at from above, this wriggle is supposed to represent the development, from simple forms of molecular transactions, of that complex form of kinesis which we call neurosis. From this point of view, all is force and energy or kinesis, and can become nothing else. Looked at from below, we have the development of consciousness. From what? We must not say from lower forms of energy or kinesis, because that involves jumping across the line, or, in other words, ignoring the distinction. From what, then? From those lower forms of "something-which-is-not-yet-consciousness-but-which-may-develop-into-consciousness," for which I have ventured to coin the term metakinesis.

I have elsewhere endeavoured to show that this view is not open to the objection that, since the kinetic sequence is a continuous and determinate one, consciousness is merely a by-product, and that an unconscious Darwin might have written and influenced the conduct of unconscious Englishmen. For consciousness, though it is distinguishable from, is, according to the hypothesis, no less inseparable from, certain complex modes of the kinetic process. As the world is constituted, such supposed kineses, separated from their metakinetic aspect, would not be the same kineses but something altogether different. In other words, it is with certain molecular transactions which have also a conscious aspect that, in the world of living beings of which we have practical knowledge, we have to deal.

It is essential that physicists and psychologists should work hand in hand. Both are endeavouring to explain the phenomena on positive lines. And if there is anything in the views that I have briefly sketched in the preceding paragraphs which runs counter to the conclusions of physics, it must go by the board, and give place to a more widely-consistent conclusion, to which physics, speaking with the voice of authority in its own special province, can give a cordial assent. C. LLOYD MORGAN.

I AM afraid that, as Prof. Lodge has accepted my "middle paragraph" so easily, he has failed to appreciate its point. For, if that paragraph is correct, the Professor's assertion, "Force is certainly necessary to direct the motion of matter," is only a truism, similar to the important geometrical theorem, "In any right-angled triangle, one angle is equal to 90°." On the other hand, Dr. Croll's assertion, to the effect that guidance is effected by "determinism," and *not* by force, is a contradiction in terms. For, by definition, that which changes motion *is* force. If, therefore, Prof. Lodge's assertion has any real meaning, he must have some independent definition of "force," and I should very much like to know what that is.

Again, Prof. Lodge in no way answers "the crux in my last paragraph." Prof. Lloyd Morgan implies in his last letter that, in the case of the sun altering the direction of motion of the earth, no energy is expended. This is, of course, only approximately true; and even in the case of his twirling his stick round his finger and thumb, as the stick is elastic, its forces of cohesion in reality do some small amount of work. It is indeed true that, if two particles were once connected by an *absolutely* inextensible string, the cohesion of the string would do no work. But what I pointed out was that, in order to bring such

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